National identity, religiosity, and religious regulation: Explaining variance in the political status and strategy of traditionally dominant religious groups

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INTRODUCTION

Around the world, the leaders of major national churches frequently invoke national identity or sovereignty when they protest against the presence of religious newcomers. In Latin America, the 1979 conference of Catholic bishops complained of an “invasion of sects” from North America. Despite gradual moves by the Vatican to encourage acceptance of other denominations in traditionally “Catholic” areas, even John Paul II complained about Protestant incursions in Latin America, and the Vatican’s apostolic delegate in Mexico urged governments to take action against American missionary agencies “preying on” Latin Americans. Brazilian bishops suggested in official documents that these missionaries were supported by the CIA (Stoll 1990, pp. 31-2).

After the fall of Communism, Orthodox clergy in the former Eastern bloc complained bitterly (and often successfully) about foreign missionaries “hunting souls.” Bishop Khajag Barsamian of the Armenian Apostolic Church told an American newspaper that “there is no reason for Armenia to accept other denominations because we have an Armenian Church and it has been in existence for many centuries . . . so why should we have other denominations?” (Billiterri 1992). The Armenian state apparently concurred; in 1999 the Chair of Armenia’s State Council for Religion told the national news agency that he was refusing to register Jehovah’s Witnesses as a religion because they were “engaged in catching souls,” an illegal practice which “in the Christian Armenia of 1700 years is unacceptable.”

The chairman concluded that:

The state and church in the Republic of Armenia are separate from each other, but for Armenia the Armenian Apostolic Church is a sacred object, and it substituted for the state for centuries. At present, our society needs the support of the Armenian Apostolic Church and the church needs the support of the Armenian state. (Noyan Tapan, 1999).
This rhetoric yields very different results across countries. In Armenia, as in Russia and in Bulgaria, states vigorously regulated against religious competitors after protests by national churches. In Latin America, similar complaints fell largely on deaf ears.

In other places, national churches rarely if ever asked for or obtained regulation to help them maintain their position against their competitors. A proliferation of Protestant missionary activity in Poland provoked little response from the Catholic hierarchy, or from the government. The Irish clergy stayed silent in 1972 when a referendum was held to remove the Catholic Church from its previous “special position” in the Irish constitution. Both churches, which still enjoy relatively high levels of attendance and piety as well as a close identification with their homelands, seem to have withstood any adverse effects of religious competition (even if other problems have hurt them).

These contrasting situations raise multiple puzzles for our understanding of traditionally dominant national religious organizations and how they maintain or lose their dominance. When do states make favorable regulations for them, giving them extensive subsidies and placing restrictions on the activities of other religious groups, and why do other states leave them to compete on their own? When do religious organizations invoke nationalism to see off their competitors, and why do some national churches fail to make use of nationalism as a resource? Why do some national churches enjoy near-monopoly with almost no state assistance at all, while others struggle to maintain their own congregations even with the full coercive backing of the state?

Current literature is poorly equipped to deal with these problems. In this paper I propose a theoretical framework for answering these questions. I argue that the significance of the traditionally dominant churches for national identity varies greatly across countries, and this rather than the size of the churches or the religiosity of populations, is what
determines whether state actors will be receptive to demands for protective regulation for the national church. Making this claim requires obtaining a cross-national measure of the fusion of religious and national identity, which has so far rarely been seen in the comparative literature but which exists in a cross-national survey instrument used by the ISSP. I also argue that not all traditionally dominant churches want or demand favorable regulations. This kind of bargaining is a costly activity for churches, and those which already enjoy high attendance and other forms of participation face diminishing marginal returns from lobbying for favorable regulation. In this way, higher levels of religiosity can lead to lower levels of religious regulation, the opposite causal relationship from that posited by the standard theory of religious markets.

This paper proceeds as follows. First I survey three bodies of literature: scholarship on the relationship between religiosity and religious regulation, scholarship on the causes of religious regulation, and scholarship on the relationship between religion and national identity. I show what theoretical insights these literatures have yielded on these major questions, and also where they leave significant gaps in our understanding. Second, I develop the theoretical framework mentioned above, outlining its rational microfoundations and empirical ramifications. Third, I provide a preliminary quantitative test of some hypotheses arising from this theory, finding some strong though far from definitive evidence that the theory is at least plausible. Fourth, I discuss what the next steps are in this research agenda.

LITERATURE REVIEW

The religious markets approach—lower regulation, higher religiosity

Scholars in the “religious markets” tradition have long argued that government regulation of religion lowers overall levels of devotion (Stark and Iannaccone 1994,
Iannaccone 1998, Stark and Finke 2000, Gill 2008). Regulation, either in the form of subsidies to favored religious groups or restrictions placed on competitors, stifles competition. In a national religious marketplace where preferences are naturally diverse, regulation makes it harder for newer, smaller and more heterodox religious groups to reach adherents who would welcome their services. Instead, government regulations push the population into the arms of a monopolistic national religion whose guaranteed market position makes it lazy and unresponsive the spiritual needs of its diverse adherents. The long-term result will be a decline in religiosity as large sections of religious demand go unmet.

In contrast unregulated religious markets promote competition, pluralism and higher levels of religious participation. Where barriers to entry into the religious market are lower, more religions will be available to provide spiritual goods to the population. Religious groups will need to compete with each other for adherents, forcing them to seek out new niches of belief and to devote their resources to meeting spiritual needs of believers rather than seeking rents from the government. In the religious markets literature scholars usually cite Western Europe as an example of the atrophying effects of government regulation, with its long-established state religions and low levels of religious participation compared to the rest of the world. They see the United States as the main example of the benefits of free religious markets, enjoying one of the highest levels of religious practice of any advanced industrial democracy.

Critics of this approach have pointed out two major problems with it. First, it is very difficult to demonstrate any relationship between religious pluralism and religious participation. Pluralism is an imperfect proxy for competition (Stark and Finke 2000) but nonetheless a positive relationship between levels of pluralism and levels of religiosity has
been a central conjecture of the religious markets argument since Finke and Stark’s (1988) landmark study of the phenomenon in nineteenth century New York State. Not only has pluralism provided most of the data available for studies of the effects of religious competition, it also gets to the heart of the argument between the religious markets approach and classical secularization theory. Berger’s (1967) “sacred canopy” argument posits that pluralism erodes religious belief by removing the link between all-encompassing religion and the web of social authority and meaning, instead leaving the individual with the responsibility of making decisions between religious alternatives. Under these circumstances, Berger argues, individuals will eventually turn away from religion altogether and toward the more rational and controllable aspects of the “disenchanted” world. The opposite effects of pluralism posited by secularization and religious markets theorists have created a considerable body of scholarly debate (Finke and Stark 1988, Breault 1989, Iannaccone 1991, Bruce 1992, Hamberg and Pettersson 1994, Stark et al 1995, Hamberg and Pettersson 1997, Olson 1999, Montgomery 2003).

However, Chaves and Gorski (2001) argue that all this scholarship has failed to produce a clear result on either side, especially the religious markets side. Given the prevalence of mixed and null empirical results, they argue that scholars should abandon any attempt to establish a general relationship between religious pluralism and participation, and should instead seek plausible conditional claims about the circumstances under which pluralism might lead one way or the other. Voas, Crockett and Olson (2002) go a step further, arguing that all empirical results on either side should be discarded because of the problems involved with using an measure of pluralism such as the Herfindahl index that has a mathematical relationship with the dependent variable of religious participation, creating a nonzero correlation between the two even where no true relationship exists. With a few
exceptions (e.g. Stark 1992), most religious markets work on the effects of pluralism suffers from this problem, and so the pluralism conjecture remains very weak.

A second major criticism of the religious markets approach is that its assumption of natural and inelastic diversity in religious demand is wrong, and leads to major deficiencies in the way markets theorists understand religious monopolies. Stark (1992), Gill (2001 and 2008) and others argue that religious monopolies cannot occur naturally without state intervention because individual religious preferences in any society will inevitably be diverse. Due to different social positions and life experiences, humans naturally have different preferences over religious strictness or liberality, worldliness or otherworldliness, traditionalism or modernism and a host of other variables that create many distinctive religious niches (Stark and Bainbridge 1985). In an unregulated market, these authors argue, no single religion could gain a monopoly position because none could possibly cater to all or even most religious demand.

Grzymala-Busse (2012) argues that natural religious monopolies do exist, citing the Catholic Churches of Poland, Ireland and Malta, all of which withstood attempts by states to break their monopoly status. She suggests the economic models of Montgomery (2003) and Barro and McCleary (2005) provide a more plausible explanation of religious monopolies; the latter demonstrate with a Hotelling model that where religious preferences across a population are more compressed (even if they remain diverse), a competitor may not profitably enter a market with high fixed operating costs dominated by a single religious provider. This leaves the question of how preferences become compressed in a national market, to which we will turn in the discussion of national identity below.

In summary, the religious markets case that less regulation leads to greater religiosity remains intuitively compelling but empirically weak. The “sacred canopy” hypothesis has
collapsed, as Berger himself acknowledges (Berger 1999). The differences between the unregulated and vigorous religious markets of the western hemisphere and the regulated, increasingly secular world of Europe continue to demand our attention, especially as the most lively religious milieus in Europe are in Poland, Ireland and Malta—countries with neither state churches (except for Malta) nor legal restrictions on minority religious competitors (Fox 2008). However, the proposed causal mechanism of pluralistic competition clearly does not hold up, as these exceptional European cases show. As Chaves and Gorski posit, the most important competition for religious vitality is probably within denominations, a hypothesis that religious markets scholars have explored but which remains relatively undeveloped.

Given the tenuous causal link between free religious markets and high religiosity, it is worth exploring whether the correlation between them suggests an opposite causal relationship—whether, and under what circumstances, high levels of religiosity lead to the development of free religious markets. Before turning to this possibility, I will survey existing explanations of the occurrence of religious regulation in the contemporary world.

**Theories of religious regulation**

The causes of religious regulation have received relatively little attention compared to the purported effects of religious regulation (as well as the religious markets literature mentioned above, see Grim and Finke 2007 for an explanation of how increased regulation leads to religious persecution). To date two major theories, both rational choice models, have emerged to explain religious regulation in a comparative way.

Barro and McCleary (2005) explain the costs of enforcing a state-backed monopoly in terms of the number of religions that would naturally arise in the absence of state
intervention. This is a function of the dispersion or compression of religious preferences across the population, as noted in the section above. State actors may have various reasons for wanting to enforce a monopoly, such as controlling the citizenry, eliminating political competition or maintaining religious peace. Whatever the state’s benevolent or non-benevolent motives for creating a religious monopoly, the likelihood that it will do so is determined in part by the costs determined by the distribution of religious preferences; the more compressed preferences are, the less costly it is to enforce a monopoly (pp. 1341-5).

In their empirical test, Barro and McCleary operationalize these costs using historical data on the market shares of countries’ major religions and the fragmentation of their denominational adherence, measured by the Herfindahl index.¹ They find that, consistent with their theory, countries with greater levels of adherence to a single “main” religion are more likely to have a state religion. They establish causality from concentrated adherence to state monopoly (rather than the other way around) by using adherence levels in 1900 as an instrument when analyzing the effects of adherence levels in 1970 and 2000.

Gill (2008) analyzes the actions of rulers seeking to regulate a religious market in terms of their political incentives. Unlike Barro and McCleary, Gill adheres to the religious markets axiom that religious preferences will be pluralistic in any society (pp. 41-2). He proposes that in order to maximize their market share under these conditions, the dominant religions in all countries will prefer government regulations that subsidize themselves and restrict minority religions. Minority religions, which could not hope to obtain such preferential treatment from the government, will always favor unregulated regimes that put

¹ Note that the use of the Herfindahl index here does not have the same problems as in the studies mentioned in section X on the effects of religious pluralism on participation. The dependent variable in this study is the presence or absence of a state church, which is not mathematically related to the Herfindahl index in the same way as measures of religious participation.
all groups on an equal footing. When no religion has a majority of adherents, all religions will seek religious freedom (pp. 44-6). Rulers will respond to these preferences according to their implications for political survival, revenue collection and the costs of ruling. Because the cheapest and least risky method of rule is to gain the ideological compliance of the population, the imperative of securing this compliance will be present in any ruler’s religion policy. In a society where one religion enjoys a dominant market share and a ruler’s grip on power is insecure, that ruler is more likely to enforce a monopoly for the favored religion in order to win the endorsement of influential religious leaders. In a more pluralistic society where large religious minorities enjoy political clout, rulers will be more likely to adopt religious freedom in order not to antagonize any important group, which could lead to a loss of revenue or political security. In any society (democratic or non-democratic) where a ruler’s grip on power is more secure, the relative bargaining power of religious groups will decline (pp. 47-55).

Gill illustrates the plausibility of this theoretical framework through a series of case studies. He shows that the commercial and revenue-raising necessity of attracting diverse refugees from religious persecution was the main driver of religious liberty in British colonial America, rather than the ideas of John Locke and the Enlightenment. Similar needs helped the cause of religious freedom in newly independent Latin American countries as commercial interests sought new trading partners from Protestant nations. The small Protestant immigrant communities gradually grew in size throughout Latin America until they posed a significant enough threat to Catholic hegemony in the mid-twentieth century that Catholic leaders successfully lobbied the dictatorships in Brazil and Argentina to impose various restrictions on Protestant mission work. By the time these countries democratized, however, Protestants made up significant enough voting blocs that they were able to ensure
the liberalization of religious markets along with other aspects of social and political life. In Russia, the organizationally decimated but still numerically dominant Orthodox Church found it difficult to compete with newcomers after Communist era restrictions on religion were lifted, and so relied heavily on political connections and nationalist rhetoric to earn very favorable treatment from the government at the expense of its most important emerging competitors. In the Baltic States a more pluralistic religious scene prevented a similar outcome.

The main empirical implication of both theories is that the higher the market share of a country’s largest religion, the more likely it is that a government seeking to minimize the risks and costs of rule will enact a regulatory framework privileging that religion and disadvantaging others. This hypothesis is very plausible but it leaves several large gaps in our understanding of why states regulate religions. First, there are many glaring counter-examples. As well as the three Catholic hegemons mentioned earlier (Poland, Ireland and Malta) many of Europe’s other least regulated religious environments feature almost one hundred percent market share for a single religion. All Scandinavian countries fit this description. We need an explanation for what seems to be systematic variance here.

This raises a second problem of how Barro and McCleary operationalize religious monopoly. The presence of an established “state church” does not necessarily indicate a serious attempt by a government to create a religious monopoly, at least not in the contemporary world. Some state-church arrangements (such as Denmark and Norway) have a far lighter footprint than others (such as Greece or Russia). The state may pay clergy and church organists in Scandinavia, but it will not make laws to restrict competitors to the state church.
The state can also regulate to the advantage of traditionally dominant religions without creating a single state church. As Stark and Finke (2000) show, despite France’s colorful anti-clerical history it continues to make laws that systematically favor the Catholic Church and disadvantage its competitors, with increasingly punitive laws for religions that are further from the traditional mainstream of French religious life. The state for example is committed to funding private schools, many of which are owned and operated by the Catholic Church. Meanwhile, nearly all religious bodies that are not members of the World Council of Churches are considered potentially “dangerous” by the government, which publicizes this danger in regular reports (p. 234). By using a binary rather than ordinal or categorical scale of religious regulation, Barro and McCleary have lumped cases of extensive regulation together with cases of very little regulation, while leaving out some other heavily regulated cases. This risks weakening genuine correlations and strengthening false ones, a situation that can be rectified by taking advantage of more detailed scales of religious regulation that were not available when Barro and McCleary’s paper was published (Grim and Finke 2006, Fox 2008).

A third problem lies in the assumption, made explicit by Gill, that dominant churches will always want regulations that disadvantage their competitors. Even if this is true, it cannot explain the enormous variance in how much traditionally dominant religions lobby for such regulations. As I have argued elsewhere (Smith 2010), different national churches have responded very differently to similar incursions by foreign competitors such as Jehovah’s Witnesses. Some have complained bitterly about the threat such interlopers pose to the nation’s soul, others have quietly accepted their presence, and others have even come to the defense of religious newcomers against laws designed to punish them. It is easy to imagine scenarios under which national churches would be more or less likely to push for
regulations of competitors. For example, a new faith that is growing rapidly would pose a much greater threat than one that is struggling to gain a foothold in the religious market, and the former would surely be more likely to prompt a traditionally dominant church to lobby for regulation (as Gill hints at in his account of Latin America, see pp. 138-9). This would mean that the relationship between market share and regulation is, at the very least, nonlinear over an important range of cases.

Both Barro and McCleary and Gill have provided important, foundational insights into the causes of government regulation of religion. Gill’s rich account of the relationship between religion and other commercial and political interests is particularly valuable, and I do not depart from his general approach of giving a central explanatory role to material interests in struggles over religion. However, if states actually bargain with dominant churches rather than making religion policy unilaterally, we need a much better account of the circumstances under which these churches actually ask for protectionist policies, thus spending political capital they could be saving for other battles.

The critical role of national identity

Comparative studies of nationalism and national identity have had relatively little to say about the relationship between religion and the nation. While there are rich ideographic literatures on this subject for many countries, according to Rieffer’s (2003) extensive survey of the comparative literature most major theorists of nationalism (Gellner, Anderson, Hobsbawm) have downplayed any role for religion in the development of the modern nation, instead focussing largely on economic factors. There have been important exceptions; Jurgensmeyer (1993) and Van der Veer (1994) both offer descriptive accounts of a specifically religious nationalism in South Asia, while Smith (1991) and Hastings (1997)
identify religion as one of the central pre-modern sources of modern national identity in Europe. However, it is only relatively recently that scholars have begun using cross-national data to unpick the tangled relationship between religion and national identity (Jones and Smith 2001, Rieffer 2003, Kunovich 2006, Voicu 2011).

Comparative scholars looking to explain religion rather than nationalism have given considerable attention to the fusion of religious and national or ethnic identities, particularly the consequences of this fusion. Secularization theorists such as David Martin (1978 and 2005) have argued that when religious and national identities fuse for historical reasons, nationalism can sustain national pockets of high religiosity in otherwise secularizing regions (see also Halikiopoulou 2011 and Voicu 2011). On the opposite theoretical side, religious markets theorists have argued that where ethnic or national conflict is closely associated with religious identity, this conflict can serve as a substitute for religious competition in a monopolistic market, thus generating high levels of religiosity where they would otherwise expect sclerotic decline (Finke and Stark 1988, Stark 1992, Stark and Iannaccone 1996, Stark and Finke 2000). In other words, scholars from both sides of the secularization-markets debate use nationalism to explain the counter-examples that pose the most vexing problems for their theories—Ireland, Poland and Quebec in particular.

Outside of the controversies over secularization and religious markets, political scientists have also begun to examine the effects of religion-nation fusion on political outcomes in which major national religions have a stake. According to Grzymala-Busse (2012) citizens of countries where fusion has taken place are more receptive to the religious framing of policy debates over divorce, abortion and same sex marriage even if they are opposed to direct political involvement of churches. She particularly stresses the importance of historical circumstances when the Church defended the nation from oppressive forces,
such as Poland under Communism or Ireland under the British Empire. In a similar vein, Htun (2003) argues that the Catholic Church in Chile exercised outlandish influence over policy debates following the end of the Pinochet dictatorship because of the political capital it accumulated as a space for civil society and political resistance during the dictatorship. The Chilean Church was alone among Southern Cone Catholic hierarchies in genuinely opposing dictatorship, and a long term political consequence is that Chile is virtually the only country in the world where divorce is illegal (pp. 102-7).

Across disciplinary fields and theoretical quarrels there is surprisingly widespread agreement that when a church comes to be seen as the defender of a people, especially against external domination, there is likely to be a fusion of religious and national identity (Sanchez 1972 pp. 91-2, Breuilly 1993 p. 29). Martin’s (1978) *General Theory of Secularization* remains the most comprehensive statement of this idea, as summarized by the following paragraph:

>(A)n indissoluble union of church and nation arises in those situations where the church has been the sole available vehicle of nationality against foreign domination: Greece, Cyprus, Poland, Belgium, Ireland, Croatia. In such countries bishops have spoken for nations and in Cyprus actually led one in the independence struggle, as well as after it. If the struggle for independence becomes self-conscious at the time of romantic nationalism then the union is a particularly potent one, with overtones of a suffering Messianic role. (p. 107)

Religious markets theorists agree, but with the critical qualification that this is not a permanent effect: continuing conflict is required to maintain this fusion and its beneficial effects for churches. They cite Quebec, the Netherlands and Poland as examples of countries that used to enjoy high religious participation because of the churches’ roles in internal nationalist conflicts, but which now see declining participation as the salience of these conflicts has diminished (Stark 1992, Stark and Iannaccone 1996, Stark and Finke 2000, pp. 239-43). Other scholars have made similar arguments; Sekulić, Massey and
Hodson (2006) show that the tie between religion and nationalism in Croatia strengthened during the mid-1990s war and weakened afterwards (p. 818).

As fruitful as the fusion between religious and national identity seems to be for explaining a number of things, cumulative science has been hamstrung by a lack of any widely used measure of this fusion. Authors do not agree on which countries count as examples of continuing fusion, and thus different scholars use the same countries to make opposed theoretical points. For example, Stark and Finke (2000) argue that nationalism continues to nourish religiosity in Ireland but not Poland (pp. 240-1), thus highlighting the importance of continuing conflict; Ramet (1998) argues that the fusion and attendant religiosity remains very strong in Poland, showing the legacy effect of the Church’s stance under communism (p. 90); and Halikiopoulou (2011) argues for the continuation of a strong fusion in Greece but not Ireland, arguing that this is because the state is increasingly reassuming the social functions of the Church in Ireland (Ch. 1).

Not only is there no standard measure of religion-nation fusion, often there is no measure at all. Because many authors in this debate concentrate almost exclusively on the effects of the religion-nation fusion rather than the phenomenon itself, they tend to take the presence or absence of the conjectured effects of fusion (e.g. high religiosity or strong denominational monopoly) as evidence of the presence or absence of fusion itself, thus failing to demonstrate causality. They also tend to ignore cases such as the United States or Russia where there is a strong case that such fusion has taken place (see e.g. McKenna 2007, Straughn and Feld 2010, Cherniavsky 1961, Knox 2005) but which do not fit within their respective causal frameworks (the US because of its lack of denominational monopoly, Russia because of its lack of religiosity).
As far as I am aware there is no comparative study using religion-nation fusion as a cross-national variable to explain levels of religious regulation, though there are some notable single country studies (e.g. Knox 2005). As Htun (2003) and Grzymala-Busse (2012) find that fusion has a positive effect on national Church success in other policy areas, we might expect a similar effect in the area of policy that arguably affects them the most, religious regulation. Indeed, we might expect that the moral authority arising from fusion would have a particularly strong political effect when it comes to the question of whether there should be a single national church that should be privileged above all others.

This question in particular demands a solid measure of the extent to which religious and national identity are fused in any country. One possible approach would be to infer fusion from the widely recognized historical processes that have created it, and to posit that religious regulation should be higher and more favorable to the hegemonic church where that church played an important defensive role against external domination. However, it is possible that the existing literature has failed to recognize alternative paths to fusion.

In Russia for example, Knox (2005), Rieffer (2003) and Ramet (1998) show that politicians since the fall of communism have benefited greatly from linking themselves to the Orthodox Church and making policy to benefit the Orthodox Church in the name of patriotism. Because Orthodox Church attendance is very low in Russia today, Russia is often left out of discussions of religious-national fusion. However, the intertwining of piety and patriotism suggests that at least some degree of fusion exists, even if it does not deliver the expected benefit of higher church attendance. This political fusion has been achieved despite the fact that the Russian Orthodox Church, while it was initially targeted for persecution by the Bolsheviks, did not play any major role in resisting Communism. Nor did it play any historical role in defending the Russian people from external domination (it was actually
associated with the Russian imperial domination of other peoples). While Russia may not
have a history of heroic church-led resistance to outside oppression, it does share with
Ireland, Poland and also the United States a historical conviction that the nation has been
specially chosen to defend Christianity (Cherniavsky 1961).

The Russian example shows the need for a measure of religion-nation fusion that is
non-binary. Rieffer argues that Russia is different from Ireland or Poland, calling it a case of
“instrumental pious nationalism” rather than “religious nationalism” (pp. 224-31).
Nonetheless, this is a difference of degree, and so we need a scale that can represent low and
moderate as well as high levels of fusion. Russia also shows the need for a measure that is
independent of either purported historical causes or purported effects, such as increased
religiosity or increased regulation.

Summary of literature review

A large body of literature across the social sciences deals directly with or has
implications for the question of religious regulation. Having surveyed the strengths and
weaknesses of this literature, three main things emerge:

1. There is a largely unexplored possibility that high religious participation causes lower
religious regulation, rather than the other way around as the religious markets
scholarship argues. The causal mechanism that markets theorists propose, pluralistic
competition, has never been convincingly demonstrated cross-nationally, and several
important cases that are both low regulation and highly religious are also
monopolistic.

2. Any account of the political dynamics that produce religious regulation requires a
more systematic treatment of the incentive structure of dominant churches, as well
as the state actors with whom they bargain. The assumption that these churches will
always want regulations that damage their competitors may well be misleading, and even if it is not it does not seem to account for variance in how these churches act.

3. Previous literature suggests that the level of fusion between religious and national identity may be important for explaining levels of religious regulation. However, in order to show causality we will need a much better and more independent measure of this fusion than has previously been seen in literature that has used it as an independent variable.

THEORETICAL FRAMEWORK

Assumptions

I follow Barro and McCleary (2005) and Gill (2008) in adopting a rational choice approach to government regulation in which the main task is determining the incentive structures of both church and state actors based on their preferences and the strategic environment. Like both authors I assume that state actors wish to ensure their political survival, minimize the costs of rule and maximize revenue, and that securing the ideological compliance of the population is the least expensive and least risky means of achieving this (a principle established by Levi, 1988).

The two sets of authors disagree over whether religious demand is naturally diverse and diffuse in any society or whether this varies with some societies having much more compressed or dispersed religious preferences than others. Here I side with Barro and McCleary, arguing that religious preferences may be much more compressed in some societies than others and thus a condition of natural monopoly is possible. Gill and his predecessors (Stark and Bainbridge 1985, Stark and Finke 2000) are probably correct that natural human diversity should give rise to diverse religious preferences, and that this is what
we would see in a situation where religious regulation has never taken place. However, all societies have histories and most of them have histories in which religious regulation has played an important role. I argue that these historical legacies of regulation can continue to shape religious preferences long after that regulation has melted away (for a useful analogy, see Nye 2007 on how eighteenth century British tariffs on French wine shaped an English preference for beer over wine that persists to this day).

Historical religious monopolies guaranteed by state regulation can compress religious preferences regardless of future regulation, though this does not always occur. Where state regulation binds religious identity to other identities it can create a condition of religious hegemony, whereby an individual cannot imagine having a different religious identity even when presented with other alternatives. Thus, even individuals with diverse preferences over levels of strictness, worldliness etc. will look to the best possible alternatives within the dominant religious body rather than outside of it. I argue that the historical circumstances under which this is most likely to happen are when religious identity became highly salient to the life chances of individuals under imperial systems in which religious separation and discrimination were important tools for rulers to manage their multinational populations. Such experiences are historically and causally prior to churches cementing their place in national identity by defending their peoples against imperial rule.

I add three further assumptions. The first is that all church-state policy results from bargains between church and state. No matter how much it may be in the interests of the state to make policy favorable to the dominant religion, the two will have never have identical interests or preferences and the state will not simply give the church what it wants without a bargaining process from which it extracts something in return.
The second is that nationally dominant churches will want to influence policy over a range of areas rather than just religious regulation. As the work of Htun and Grzymala-Busse shows, major churches try to exercise influence over many social and even economic policies. In Catholic countries for example the Church has lobbied against divorce, abortion, same sex marriage, the death penalty and stem cell research, and in favor of nuclear disarmament, workers’ rights and immigrants’ rights. The very different levels of emphasis the Church places on these issues in different countries shows the importance of national political context for determining Church lobbying activity, even where doctrine is invariant (Grzymala-Busse 20??). Churches pick their battles.

This brings us to a third assumption, which is that political lobbying is a costly activity for churches. Lobbying in general is expensive and consumes resources that could be productively used elsewhere (see Tollison 1982). For churches it involves not only the costs of employing lawyers and spokespeople, but also the costs of raising certain issues—whether in discussions with state officials or in sermons to its own adherents—when it could be talking about other things. It follows from this that any political lobbying done in one area presents an opportunity cost in terms of the lobbying that could be done in another area.

**Theoretical argument**

Determining the extent to which any state will regulate for the benefit of a dominant religion requires us to explain two things: how much the dominant religion is likely to lobby for subsidies for itself and restrictions on its competitors, and how responsive a state is likely to be to this lobbying given its own imperatives of political survival, raising revenue and minimizing the costs of rule.
Church lobbying for religious regulation

The type of regulation under discussion here—subsidies to dominant churches and various restrictions on the activities of their competitors—is designed to ensure the organizational health of major national churches, guaranteeing their income and preventing them from losing adherents to religious competitors. Given the opportunity costs associated with lobbying for such regulation, a dominant church’s calculus about whether to lobby for it will be based on its assessment of how organizationally healthy it will be without more favorable regulations. Churches that expect to flourish even in the face of competition are much less likely to lobby for favorable regulations than those that feel under threat from competitors or from other social forces that are sapping their strength.

I argue that the best measure religious organizations have of their own vitality is the rate of participation by their nominal adherents. Adherent participation in the form of attendance at services and volunteer activities is the lifeblood of any religious organization, providing money and man-hours (see Iannaccone 1998). Religious groups that lack substantial participation will have to rely more on the state for income. For religious groupings with a majority of the population as nominal adherents, this concern about the participation of their own members is prior to concerns about nominal market share. Churches with high levels of participation are unlikely to lose many members to religious competitors, and can remain strong even if they lose some. However, for churches with low levels of participation, the presence of competitors is a much more serious concern. Competitors are more likely to pick up the “unchurched” members of the dominant religion (see Gill 2008, p. 13) and if they succeed in converting participant members of the dominant religion, the marginal loss is that much greater if there are fewer participants.
Therefore, I posit that dominant churches with high levels of participation are much less likely to lobby for favorable regulations, especially restrictions on competitors, than churches with lower levels of participation. Because, as mentioned above, national churches want to exercise influence over a range of policy domains, they must be careful about how they use their political capital. Even in highly religious countries where the national church is well-regarded, citizens may be wary about clerical interference in politics (Grzymala-Busse 2011). Churches that interfere too much and on too many issues risk backlashes that could alienate their political partners. Churches which are relatively secure in their organizational health do not need to seek state regulation for their benefit, but they may well need to spend political capital in order to produce other policy outcomes such as restricting abortion or divorce. Depending on how important they believe these issues are and the likelihood of achieving a favorable outcome without extensive lobbying, churches are much more likely to prioritize these issues, where they actually need to fight.

This argument does beg the question of why religious participation in major churches varies from country to country. I concur with the religious markets literature that this is depends on the ability of these churches to serve the spiritual needs of their members, but for the reasons outlined in the literature review above I do not believe this is a function of regulation or competition. Religious revivals prompted by organizational reform and changing church priorities can occur in highly regulated environments (see Larkin 1972 on nineteenth century Ireland) or practically unregulated ones (see Howe 2007 on America’s second great awakening). If, however, regulation does play a role in levels of participation, this suggests the possibility of a reinforcing effect: participation in the major religion would drop in regulated environments, prompting it to lobby for more regulation (or at least to resist any relaxation of regulations).
There is also the possibility, as discussed further above, that nationalism can promote high levels of religious participation (Voicu 2011), particularly when national identity is fused with religious identity (Martin 2005). As I discuss below, churches which enjoy a strong association with national identity also have considerable political clout compared to those which do not. According to my argument, churches whose participation levels are driven by a strong sense of national identity should not feel the need to lobby against competitors. However, there appear to be situations, especially in the Orthodox Christian world, where a strong sense of national identity has been unable to increase participation—it is primarily in this part of Europe that Voicu finds no association between nationalism and religious participation. As I discuss further below, it is in these situations that we should expect to find the highest levels of religious regulation.

*State responsiveness to lobbying for favorable religious regulation*

National identity has been the world’s most pervasive and powerful citizen loyalty for the last century (Smith 1979). As discussed in the literature review, many authors have attributed powerful effects to the fusion of religious and national identity. I argue that we should expect instances of religion-nation fusion to increase the state’s receptiveness to demands for favorable regulation from the dominant religion. Nationalism as Gellner (1983) argues is a principle of political legitimacy, that states are legitimate when their borders are congruent with those of distinctive nations. The more that national identity is bound to a particular religion or denomination, the more citizens are likely to want or accept regulations designed to maintain the Christian (or Catholic, or Orthodox, or Muslim) character of the country. Thus it is least costly and most beneficial for state actors to make such regulations
when the citizenry sees national identity as inseparable from the major religion or denomination.

The religious content of national identity is, I argue, more important than the major religion’s participation levels or market share in determining the state’s receptiveness to its lobbying for regulation (or against deregulation). As Knox’s (2005) case study of Russia and Halikiopoulou’s (2011) case study of Greece suggest, nationalism can give a low-attendance church political power that goes well beyond those followers who actually listen to clergy regularly. Especially in a time of new independence or some other upheaval when old national identities are being destroyed and rebuilt, political entrepreneurs are likely to gravitate to symbols that resonate with historical national identification, even if they have lost all practical significance for much of the population (see Verdery 1999).

It is possible to have very strong national identification with an organizationally weak national religion, though the inverse is almost never seen. This suggests that the nation-religion fusion is a necessary but not sufficient condition for high religious participation, which can in turn dissuade dominant churches from lobbying for more regulation. This means the pattern we should expect to see is that where high religion-nation fusion has created a culture of strong religious participation there will be relatively low levels of favorable regulation for dominant churches, because these churches will have little incentive to bargain for such regulations or against deregulation. However, where high religion-nation fusion is not accompanied by strong participation, we should expect to see higher levels of religious regulation, as dominant churches will expend more of their resources lobbying for such regulation, and will be likely to obtain it from state actors seeking to maintain their legitimacy and popularity. Where both religion-nation fusion and participation are low we
should expect to see relatively little regulation. Churches may lobby for favorable treatment, but state actors will see little benefit in granting it.

In a notable passage in *The Political Origins of Religious Liberty*, Gill notes with skepticism the claims of national churches that regulation is necessary to maintain national integrity:

Leaders of historically dominant religions in a nation often resort to claims that a nation’s populace subscribes only to one true faith and that prohibitions on foreign sects are required to protect the citizenry from cultural contamination. This raises an interesting dilemma. If one religion truly defines a national culture, and people are steeped in that culture, restrictions on foreign missionaries would be unnecessary; the populace would reject the new sect out of hand. In reality, such restrictions are often necessary because there is a variety of preferences for different types of religion in a society and because the dominant church has not done a sufficient job of capturing the loyalty of the citizenry, leaving the “unchurched” ripe for the picking. (p. 13)

I agree with Gill’s skepticism, but I wish to offer a more complete statement of it: in some countries, for all intents and purposes the populace really does subscribe to “one true faith,” and restrictions on foreign missionaries are unnecessary. In these countries, such as Ireland and Malta, we do not see national churches clamoring for restrictions. When dominant churches make the kind of nationalist appeals that Gill describes, it is a sign of weakness.

**EMPIRICAL TESTING**

**Hypotheses**

From the discussion above I derive the following testable hypotheses:

_Hypothesis 1:_ Government regulation will be more favorable to the largest religion and discriminatory against others where that religion enjoys a close association with national identity.

_Hypothesis 2:_ Majority religions with high levels of participation will be less likely to bargain for favorable regulations or against deregulation, thus resulting in lower levels of regulation where majority religious participation is higher.
Hypothesis 3: Where the dominant religion has both high participation and a strong association with national identity, we should see relatively low levels of regulation. This is because governments will not make favorable regulations for the dominant religion without bargaining, and the dominant religion will be much less likely to enter into such bargaining when its participation is high.

I derive two more hypotheses from existing rational choice literature on religious regulation:

Hypothesis 4: The more fragmented the religious market (i.e. where no single group commands a majority) the less likely that the government will enact legislation favoring any particular group.

Hypothesis 5: The larger the market share of the largest religion, the more likely it is that the government will make regulations favorable to that religion and discriminatory against others.

Data and measures

In order to test these hypotheses I have assembled a small cross-national dataset from a variety of sources. Selection of the 33 countries (listed in Table 3 in the appendix) was determined solely by their inclusion in the International Social Survey Project’s National Identity II survey of 2003. The main limitation of this set of countries is that they are nearly all democracies (with the partial exception of Russia) and nearly all have Christian majorities (with the exceptions of Japan, Taiwan and Israel). This may limit the generalizability of any findings beyond the democratic world or the Christian world, but the benefit of having relatively similar countries in the dataset is that it provides built-in controls that allow us to see true correlations more clearly.

The dependent variable in this dataset is government regulation of religion, which I measure here using Jonathan Fox’s index of government involvement in religion (GIR) between 1999 and 2003. To this point, Fox’s (2008) data remains the most comprehensive cross-national data in existence regarding this subject. It is important to note two things about this data. First, what Fox calls “government involvement,” including all forms of
subsidies, interventions and restrictions, is conceptually identical to what religious markets theorists call “regulation.” Gill (2008) makes the plausible argument that any regulation designed to encourage one religion hurts competitors by giving it an advantage over others in the religious marketplace. Second, Fox classifies all cases in this dataset as instances of either “Accommodation” of religion, “cooperation” with religion, the establishment of “one official religion,” or the creation of a “civil religion” (an unofficial state religion). In all these cases, whatever regulation exists benefits either the major religion of the country or a group of major religions (some of which may be quite small, but are recognized as being important to a country’s religious life). There are no cases in this data of “hostile” religious regulation, where the purpose is to crush religion in general and the main religion in particular.

For the independent variable of the fusion of religious and national identity, I use a survey instrument from the ISSP’s National Identity II survey of 2003 made available by GESIS—Leibniz Institute for the Social Sciences. The question I use (v15) is part of a series of questions asking “Some people say that the following things are important for being truly [NATIONALITY]. How important do you think each of the following is?” v15 asked about religion—whether it was important to be a member of that country’s dominant faith. In some multidenominational countries (the US, Canada, Australia, Germany) the religious grouping mentioned was “Christian” while in others a specific denomination was mentioned (e.g. Catholic for Ireland). For each country, I took the total number of people who said this identity was “very important” or “somewhat important.” The benefit of using this variable is that it is the only one I am aware of that provides a uniform measure for the intertwining of religious and national identity, separate from any purported causes or effects. As far as I am aware, however the only other study that makes use of it is Kunovich (2006). The main drawback, as mentioned earlier, is that it limits the analysis to 33 cases.
For the independent variable of religious participation I used the same survey’s question about how often respondents attend religious services. To construct my measure I took the total percentage for each country of respondents who claimed to attend church 2-3 times a month or more. It is possible that respondents are not entirely honest when they answer this question due to social desirability pressures, and that respondents in countries where religion is important to national identity will be more likely to overestimate their attendance. Any compression of distance between these two quantities would be likely to bias my results conservatively, so I am comfortable with this potential problem.

My estimates of the largest religion’s share of the population in 2000 and the religious Herfindahl index in 2000 comes from Robert Barro’s Religion Adherence Dataset (available at http://rbarro.com/data-sets/), which he derived from Barrett, Kurian and Johnson’s World Christian Encyclopedia (2003). The Herfindahl index, widely used in social scientific studies of religion, is an estimate of the probability that any two random members the population would belong to the same religious group.

I include a control variable for countries’ general levels of state social regulation, measured here by the Freedom House average score for 2003 (available at http://www.freedomhouse.org/template.cfm?page=363&year=2003). Nearly all countries in this dataset are at the “more free” end of the scale, i.e. 1 or 2. Finally, I include dummies for whether Christian countries are predominantly Catholic, Protestant or Orthodox. I include this because each of the three traditions has different norms regarding religious attendance (see Voicu 2011 and Halikiopoulou 2011) and I wanted to avoid conflating the effect of attendance with a more general denominational effect for national regulation (as proposed by Fox 2008, Chapter 4). However, we should also consider the possibility that these different denominational norms around attendance cause denominational effects for
regulation. Because, for example, there are very low levels of voluntary religious participation in the Orthodox world, this naturally makes autocephalous Orthodox churches more reliant on states.

Descriptive statistics for all variables are shown in Table 1 below, and the complete scores for all countries are given in Table 3 in the appendix.

[Table 1 about here]

**Estimation**

I use three slightly different OLS models to test my hypotheses. The first is a straightforward linear model that leaves out the three denominational dummies. The second is then same model but with the dummies included. The point of including both is simply to observe how important these denominational controls are, if at all. In both of these models, the relationship between national identity and religious participation is additive. I argue that participation will have the opposite effect of regulation from national identity, and thus the presence of the former will simply subtract from the presence of the latter. However, it is also possible to conceive of them multiplicatively, that is that participation only effects regulation by modifying the effect of national identity. Thus I also include an interactive model that represents the relationship this way. Because of the small sample size, I bootstrapped each regression with 500 replications. The results are shown in Table 2 below.

[Table 2 about here]

**Results**

These results generally support my hypotheses and cast doubt on the alternatives. In both of the additive models, the percentages of people who say religion is important to
national identity and who attend services regularly are large and significant predictors of levels of religious regulation. For every percentage increase in people who say religion is important to national identity, religious regulation rise by .3 on Fox’s GIR scale. This means that a standard deviation increase in the national identity variable leads to about three quarters of a standard deviation increase in the religious regulation variable. The percentage of people attending church services regularly has, as predicted, almost the complete opposite effect, with a .37 decrease in GIR for every percentage increase. In terms of standard deviations, this is an almost exactly equal but opposite effect to the percentage saying religion is important to national identity. This is despite the fact that the two things so frequently go together, as Table 3 goes. This strongly supports my theory.

In the multiplicative model, as expected the percentage of attendance has no independent effect, though the independent effect of national identity remains large and significant. The marginal effects of percentage attendance as a modifying variable on national identity are shown in Figure 1 below:

[Figure 1 about here]

Attendance is on the X axis and the national identity variable is on the Y axis. At the minimum of church attendance, the coefficient of the national identity variable is at about 3.5, while at the maximum of church attendance it is only around .1. When regular attendance reaches about 35% (the 75th percentile of the sample) it ceases to be significant. This supports my theory that countries with high religion-nation fusion are less likely to have high regulation when their majority churches are well attended.

In none of the models is the Herfindahl index or the majority religious share significant. This suggests that the extent of religion-nation fusion is much more politically salient than nominal numbers of adherents or the simple concentration of the religious
market. Including the denominational dummies slightly increases the $R^2$ (from .54 to .57) but otherwise has no real effect in this model, and none of them approach significant explanatory power on their own.

**DISCUSSION AND CONCLUSION**

I intend this empirical demonstration to be the opening salvo rather than the definitive test of my theory. As it stands, this test has some severe limitations (apart from the small degrees of freedom). Perhaps most importantly, it is not a direct test of my proposed causal mechanism for why high church attendance leads to lower regulation. All I have shown here is that there is a relationship between the two in the expected direction. To properly test my theory, we need to compose systematic data on church lobbying over religious regulation that has taken place in these countries. This is an inherently interesting project: how are churches different from or similar to other industries that lobby the government for more or less competitive marketplaces? These results provide important theoretical motivation for this next stage of data collection and development.

The second limitation is multiple endogeneity problems. As yet I lack any instrumental variables to separate out the possibly mutually causal effects of religious attendance and religious regulation and religion-nation fusion and religious regulation. I am not sure what form such instruments would take, and I welcome any suggestions.

For now, I have offered an alternative way of thinking about religious regulation and religious participation from a political economy perspective. While there are reasons to believe that low religious regulation promotes high religiosity through market mechanisms, I hope to have shown that it is at least plausible that high religiosity can promote low religious regulation through political mechanisms. Furthermore, I have provided evidence that the
extent of fusion between religious and national identity is at least as politically important at the size of major religions or the fragmentation of the religious market. By locating a solid cross-national measure of this fusion we may begin to investigate its effects more cleanly, instead of relying on proxies tied either to its putative causes or effects. The ISSP data suggests that contrary to the convictions of religious markets theory, a strong fusion between religious identity and nationalism exists in many places where there is no continuing conflict to sustain it. Discovering exactly what sustains this relationship is another important task for future research.


Appendix

Table 1: Descriptive statistics of major variables

<table>
<thead>
<tr>
<th></th>
<th>Mean</th>
<th>Std. Dev.</th>
<th>Min.</th>
<th>Max.</th>
</tr>
</thead>
<tbody>
<tr>
<td>Regulation index (GIR)</td>
<td>17.48</td>
<td>10.5</td>
<td>0</td>
<td>37</td>
</tr>
<tr>
<td>% Say religion important to national identity</td>
<td>45.12</td>
<td>21.15</td>
<td>13</td>
<td>84</td>
</tr>
<tr>
<td>% Majority religion regular attendance</td>
<td>28.12</td>
<td>21.38</td>
<td>5</td>
<td>84</td>
</tr>
<tr>
<td>Herfindahl index</td>
<td>.50</td>
<td>.20</td>
<td>.23</td>
<td>.85</td>
</tr>
<tr>
<td>Fraction of largest rel. share of population</td>
<td>.63</td>
<td>.21</td>
<td>.22</td>
<td>.92</td>
</tr>
<tr>
<td>Average Freedom House score 2003</td>
<td>1.4</td>
<td>.84</td>
<td>5</td>
<td>1</td>
</tr>
</tbody>
</table>
Table 2: Results of OLS estimations (dependent variable is GIR index of religious regulation)

<table>
<thead>
<tr>
<th></th>
<th>Model I</th>
<th></th>
<th>Model II</th>
<th></th>
<th>Model III</th>
<th></th>
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<tbody>
<tr>
<td></td>
<td>coeff.</td>
<td>$p$ value</td>
<td>coeff.</td>
<td>$p$ value</td>
<td>coeff.</td>
<td>$p$ value</td>
</tr>
<tr>
<td>% saying major religion is important to national identity</td>
<td>.30</td>
<td>.005</td>
<td>.29</td>
<td>.035</td>
<td>.35</td>
<td>.008</td>
</tr>
<tr>
<td>% of major religion regularly attending services</td>
<td>-.37</td>
<td>.000</td>
<td>-.38</td>
<td>.001</td>
<td>-.19</td>
<td>.450</td>
</tr>
<tr>
<td>Multiplicative interaction of first two variables</td>
<td>-</td>
<td>-</td>
<td>-</td>
<td>-</td>
<td>-.002</td>
<td>.483</td>
</tr>
<tr>
<td>Herfindahl index</td>
<td>33.51</td>
<td>.353</td>
<td>40.91</td>
<td>.333</td>
<td>34.69</td>
<td>.364</td>
</tr>
<tr>
<td>Adherents of major religion as fraction of population</td>
<td>-15.89</td>
<td>.660</td>
<td>-23.32</td>
<td>.587</td>
<td>-16.24</td>
<td>.669</td>
</tr>
<tr>
<td>2003 Freedom House average score</td>
<td>-.74</td>
<td>.815</td>
<td>-.50</td>
<td>.900</td>
<td>-.50</td>
<td>.862</td>
</tr>
<tr>
<td>Predominantly Catholic dummy</td>
<td>-</td>
<td>-</td>
<td>5.39</td>
<td>.442</td>
<td>-</td>
<td>-</td>
</tr>
<tr>
<td>Predominantly Protestant dummy</td>
<td>-</td>
<td>-</td>
<td>3.95</td>
<td>.588</td>
<td>-</td>
<td>-</td>
</tr>
<tr>
<td>Predominantly Orthodox dummy</td>
<td>-</td>
<td>-</td>
<td>7.74</td>
<td>.476</td>
<td>-</td>
<td>-</td>
</tr>
<tr>
<td>Constant</td>
<td>8.66</td>
<td>.162</td>
<td>5.58</td>
<td>.595</td>
<td>4.73</td>
<td>.554</td>
</tr>
<tr>
<td>$R^2$</td>
<td></td>
<td>.53</td>
<td></td>
<td>.57</td>
<td></td>
<td>.54</td>
</tr>
</tbody>
</table>
Table 3: Country scores for variables

<table>
<thead>
<tr>
<th>Country</th>
<th>GIR</th>
<th>% religion important to nat. id.</th>
<th>% maj rel. regularly attend</th>
<th>Herf. Index</th>
<th>% largest rel. share</th>
<th>Freedom House av. 03</th>
<th>Largest denom.</th>
</tr>
</thead>
<tbody>
<tr>
<td>Australia</td>
<td>3</td>
<td>37</td>
<td>23</td>
<td>0.23</td>
<td>0.32</td>
<td>1</td>
<td>Protestant</td>
</tr>
<tr>
<td>Austria</td>
<td>24</td>
<td>53</td>
<td>22</td>
<td>0.58</td>
<td>0.76</td>
<td>1</td>
<td>Catholic</td>
</tr>
<tr>
<td>Bulgaria</td>
<td>37</td>
<td>77</td>
<td>7</td>
<td>0.54</td>
<td>0.72</td>
<td>1.5</td>
<td>Orthodox</td>
</tr>
<tr>
<td>Canada</td>
<td>4</td>
<td>54</td>
<td>48</td>
<td>0.24</td>
<td>0.39</td>
<td>1</td>
<td>Catholic</td>
</tr>
<tr>
<td>Chile</td>
<td>20</td>
<td>54</td>
<td>28</td>
<td>0.5</td>
<td>0.67</td>
<td>1.5</td>
<td>Catholic</td>
</tr>
<tr>
<td>Czech R.</td>
<td>19</td>
<td>29</td>
<td>23</td>
<td>0.34</td>
<td>0.4</td>
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<td>Catholic</td>
</tr>
<tr>
<td>Denmark</td>
<td>26</td>
<td>33</td>
<td>6</td>
<td>0.78</td>
<td>0.88</td>
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<tr>
<td>Finland</td>
<td>33</td>
<td>23</td>
<td>7</td>
<td>0.73</td>
<td>0.85</td>
<td>1</td>
<td>Catholic</td>
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<tr>
<td>France</td>
<td>23</td>
<td>18</td>
<td>18</td>
<td>0.49</td>
<td>0.67</td>
<td>1</td>
<td>Catholic</td>
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<tr>
<td>Germany</td>
<td>20</td>
<td>37</td>
<td>30</td>
<td>0.28</td>
<td>0.36</td>
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<tr>
<td>Hungary</td>
<td>23</td>
<td>43</td>
<td>11</td>
<td>0.44</td>
<td>0.61</td>
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</tr>
<tr>
<td>Ireland</td>
<td>16</td>
<td>58</td>
<td>72</td>
<td>0.73</td>
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<td>Catholic</td>
</tr>
<tr>
<td>Israel</td>
<td>37</td>
<td>84</td>
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<td>Japan</td>
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<td>0.41</td>
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<td>0.35</td>
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<td>5</td>
<td>0.81</td>
<td>0.9</td>
<td>1</td>
<td>Protestant</td>
</tr>
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<td>Philippin.</td>
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<td>84</td>
<td>72</td>
<td>0.55</td>
<td>0.73</td>
<td>2.5</td>
<td>Catholic</td>
</tr>
<tr>
<td>Poland</td>
<td>22</td>
<td>75</td>
<td>68</td>
<td>0.85</td>
<td>0.92</td>
<td>1.5</td>
<td>Catholic</td>
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<td>Portugal</td>
<td>28</td>
<td>66</td>
<td>34</td>
<td>0.79</td>
<td>0.89</td>
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<td>Russia</td>
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<td>58</td>
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Figure 1: Marginal effects graph of % religious attendance (X) as modifying variable on % saying religious identity is important to national identity (Y), effects on GIR.